Cash transfers, climatic shocks and resilience in the Sahel

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Abstract

Policy makers are increasingly interested in strategies to promote resilience and mitigate the effects of future climatic shocks. Cash transfer programs have had widely documented positive welfare impacts. While cash transfers are often used to protect households against shocks, their role in fostering resilience has been less studied. This paper assesses whether small regular cash transfers strengthen poor households' ability to mitigate the welfare effects of drought shocks. It analyzes mechanisms through which cash transfers contribute to resilience, including savings, asset accumulation or income smoothing in agriculture and off-farm activities. It combines household survey data collected as part of a randomized control trial in rural Niger with satellite data used to identify exogenous rainfall shocks. The results show that cash transfers increase household consumption by about 10 percent on average. Importantly, this increase is mostly concentrated among households affected by drought shocks, for whom welfare impacts are larger than transfer amounts. This result is explained by households' increased ability to protect earnings in agriculture and off-farm businesses when shocks occur. Findings show that multiyear cash transfer programs targeting poor households can effectively foster resilience by facilitating household savings and income smoothing.

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1 Introduction

Policy makers are increasingly interested in strategies to promote resilience by enhancing households' ability to prepare and protect themselves against shocks. The issue has become salient given widespread concerns about climate change and the growing frequency of shocks globally. The focus on improving resilience is also at the center of efforts to better coordinate the nexus between emergency humanitarian interventions and more permanent development programs (Clarke and Dercon, 2016; Bowen et al., 2020). However, there is little evidence on effective policy options to improve poor households' resilience.

Cash transfers have been widely considered as a policy instrument to improve poor people's lives. A large number of studies show that cash transfers improve welfare and facilitate human capital accumulation (Fiszbein and Schady, 2009; Ralston et al., 2017). Poor households also invest a substantial part of the transfers in productive activities (Bastagli et al., 2016; Gertler et al., 2012; Daidone et al., 2019; Stoeffler et al, 2019). Although cash transfer interventions often aim to protect households against shocks, there is limited evidence on the linkages between cash transfers and resilience. Yet, the degree to which cash transfers contribute to strengthening resilience can have important implications for the design of social protection systems. It can affect key design parameters (such as transfer amounts or duration), or the arbitrage between alternative social protection instruments (Ikegami et al., 2017), such as the balance between multi-year programs with a role to offer protection against future risk and shock-responsive programs rolled out after shocks occur.

In this paper, we test whether cash transfers help households mitigate the welfare effects of climatic shocks. We also analyze mechanisms that contribute to enhance resilience, including the role of savings, asset accumulation and income smoothing in agriculture and off-farm household enterprises. We study a government-led cash transfer program that delivers small monthly unconditional transfers of 10,000 CFA (about 20 USD)[1] for two years. The program is part of a government-led national safety net and targets poor households in rural shock-prone areas of Niger.

Shocks contribute to poverty persistence and limit upward mobility (Carter and Barrett, 2006; World Bank, 2013). Realized shocks can have lasting consequences on a broad range of outcomes, including health or education (Alderman et al., 2006; Maccini and Yang, 2009; Wilde et al., 2020; Mullins & White, 2020). To cope with income shocks, households at times employ costly strategies such as cutting consumption, migrating, or depleting physical or human capital (Chetty & Looney, 2006; Marchiori et al., 2012; Burke et al., 2014; Carter et al., 2007; Deuchert & Felfe, 2015; Janzen and Carter, 2018). In addition, the mere existence of risk (ex-ante) pushes households to adopt costly, low-risk strategies that reduce income variability at the expense of forgoing higher-risk higher-return activities (Elbers et al., 2007; Stoeffler et al., 2020; Zimmerman and Carter, 2003). The large welfare costs of risk and realized shocks reflect imperfections in insurance markets and households' risk-management mechanisms (Jalan and Ravallion, 1999; World Bank, 2009). Climate change will likely amplify these costs (Kalkuhl & Wenz, 2020).

The concept of resilience broadly characterizes the capacity to resist and recover from shocks. Barrett and Constas (2014) define "development resilience" as "the capacity over time of a person, household or other aggregate units to avoid poverty in the face of various stressors and in the wake of myriad shocks". While its merits and novelty with respect to earlier work on

[1] The exchange rate was 1 USD = 497 CFA francs (CFA) on January 1, 2013, and 1 USD = 541 CFA francs (CFA) on January 1, 2015. Yearly transfers amount to 12% of total consumption on average.

"vulnerability" have been debated, the concept of resilience is now at the core of efforts to better articulate the nexus of humanitarian and development programs (FAO, 2016; Béné et al., 2017; Serfilippi and Ramnath, 2018). It has also renewed attention on poverty dynamics and the role of shocks (Béné et al., 2012). At the same time, resilience measurement is challenging and ideally relies on long-term or high-frequency panel data sets (Béné et al., 2017; Cissé and Barrett, 2018; Upton et al., 2016). Recent empirical studies have attempted to construct resilience indicators, assess factors associated with household resilience or understand whether suggested resilience indicators correlate with greater well-being or capacity to cope with shocks (Alfani et al., 2015; d'Errico et al., 2018; Knippenberg et al., 2019; Smith and Frankenberger, 2018).

In this paper, we study how cash transfers affect households' ability to mitigate the welfare effects of shocks. Specifically, we measure whether cash transfer beneficiaries hit by a climatic shock are better able to smooth consumption compared to cash transfer non-beneficiaries hit by the same shock. We also consider mechanisms through which households achieve consumption smoothing and become more resilient, including savings, asset accumulation and income smoothing in agriculture and off-farm household enterprises. While we are not capturing resilience in a fully dynamic perspective (which would require longer panels), our approach is consistent with the frameworks from Barrett and Constas (2014), Constas et al. (2013) or Béné et al. (2017). It is also in line with empirical approaches to analyze the role of safety nets in improving households' risk-management and income smoothing by analyzing heterogeneity in impacts by exposure to climatic shocks (Macours et al., 2012).

Various types of interventions aim to foster resilience. One approach has been to set up integrated programs that address sources of stress (such as land degradation) while strengthening the capacity of households and communities to respond to shocks, for instance through cash-forworks programs complemented by additional interventions (Béné et al., 2017). While more limited in scope, cash transfers can contribute to strengthen household resilience in two important ways. First, as social safety nets, regular cash transfers can contribute to protect households from income fluctuation, prevent the use of adverse coping mechanism such as asset depletion, or sharp drops of consumption. Second, cash transfers allow households to accumulate productive assets and diversify their income portfolio (Gertler et al., 2012; Stoeffler et al., 2019). An improved asset base or greater diversification can in turn decrease the need for households to reduce consumption by smoothing income when shocks occur (Macours et al., 2012), thus making them more resilient (Barrett and Constas, 2014; Cissé and Barrett, 2018). These mechanisms are at the core of the rationale for social safety net programs (Grosh et al., 2008; Beegle et al., 2018).

Few studies have measured directly how social safety nets contribute to household resilience.[2] In Ethiopia, Dercon and Krishnan (2004) find that food aid helps households smooth consumption, while Béné et al. (2012) find limited impacts of the Productive Safety Net Program on the capacity of households to cope with severe shocks (consistently with other impact evaluations of the program). More closely related to our research, two experimental studies combine survey and satellite data to measure whether cash transfers protect beneficiaries from exogenous climatic shocks. Macours et al. (2012) show that cash transfers in Nicaragua improve household capacity to cope with future climatic variability by facilitating income smoothing when combined with complementary interventions such as vocational training or cash grants. However, cash transfers alone are not found to help protect households against future shocks. On the other hand, Asfaw et

[2] For instance, Beegle et al. (2018) state that safety nets "increase resilience" in Africa (chapter 2) because impacts on household investments and assets are found, but actual household responses to shocks are not observed.

al. (2017) find that cash transfer beneficiaries are able to protect their consumption and food security from rainfall deviations in Zambia, but the mechanisms through which this occurs are not studied. Overall, the evidence is limited and mixed, indicating that the impact of cash transfers on resilience may work through different channels, which need to be better understood.

We contribute to this literature by studying a government-led cash transfer program that delivers regular, small, unconditional cash transfers to rural Nigerien households for two years. Niger is a particularly relevant setting given it has high poverty levels, was ranked last in 2018 in terms of human development indicators, faces frequent weather shocks, and has rolled out a safety net program as a flagship government initiative.[3] The expectation was that regular cash transfers delivered year-round would help households protect themselves against shocks and promote resilience. We analyze household data collected for the randomized evaluation of the project between 2012 (baseline) and 2015 (follow-up) to measure the impact of the transfers.[4] We combine household surveys with satellite data on rainfall to identify whether cash transfers mitigate the welfare effects of exogenous climatic shocks on household consumption and food security. We also measure impacts on income-generating activities, savings and assets to assess pathways to enhanced resilience.

In doing so, we make three main contributions to the literature on resilience and cash transfers. First, we document the impact of government-led, multi-year unconditional cash transfers in an extremely poor context where households face high exposure to climatic shocks.[5] Second, we

[3] Despite its low development indicators, Niger is also under-studied (Porteous, 2020).
[4] Specifically, the paper extends the analysis of an RCT embedded in the first phase of the national safety net program and presented in Premand and Barry (2020), who disentangle the relative effects of cash transfers and behavioral change accompanying measures on young children's human capital. Premand and Barry (2020) also provide more information on the study background, implementation, and collaboration with government. We use the baseline and follow-up data collected as part of the RCT.
[5] We also compare our results with those found in the study by Stoeffler et al. (2019) of a prior, smaller-scale cash transfer pilot project in Niger.

focus explicitly on household resilience by measuring how cash transfers protect consumption and food security from climatic shocks. By studying intermediary outcomes such as savings, asset accumulation, and income smoothing through household economic activities, we contribute to a better understanding of the mechanisms through which cash transfers foster resilience. Finally, we add to the existing literature by analyzing the mitigation role of cash transfers based on recent advances in resilience measurement, such as the metrics introduced by Cissé and Barrett (2018).[6]

Our results show that cash transfers improve household welfare and food security. Importantly, these results mostly stem from households affected by shocks, for whom welfare impacts are larger than transfer amounts. While droughts induce large welfare losses, cash transfers fully mitigate these losses and protect consumption levels. Several mechanisms contribute to enhance resilience. Cash transfer recipients are more likely to participate in saving groups and save more. They are also more likely to be able to smooth income from agricultural activities and non-agricultural household enterprises when exposed to shocks. On the other hand, we find limited impacts on asset accumulation with few differences in household durables or livestock. Overall, the findings show that cash transfers improve households' capacity to protect their income from shocks, which in turn explains the magnitude of the welfare impacts from cash transfers among households affected by droughts.

[6] To our knowledge, only one recently published article measures the impact of an intervention on a resilience metric (using the Cissé-Barrett approach), and this intervention was a lumpy asset transfer program, not a safety net program (Phadera et al., 2019).

The paper is structured as follows. Section two presents the context and the data. Section three describes our methodology. Section four presents the results. Section five concludes and discusses some implications.

2 Context and data

2.1 Safety Nets in Niger

Niger is one of the poorest countries in the world, with a GDP per capita of 895 USD in 2015 (in constant 2011 USD) and a poverty rate of 51.4 percent in rural areas (World Bank, 2016). Employment is highly concentrated in subsistence (rain-fed) agriculture, in which more than 80% of the labor force is engaged. Located in the semi-arid Sahel region on the edge of the Sahara Desert, Niger's rural population is highly exposed to climatic shocks such as drought. The incidence and severity of these shocks is expected to increase due to climate change (World Bank, 2009; World Bank, 2016). Cereal crops such as millet and sorghum are the most common, and rainfall variation is correlated with agricultural production. Agricultural production is constrained by a short rainy season, and very limited access to irrigation. This contributes to substantial seasonal food insecurity (Schnitzer, 2019). In addition to poverty and food insecurity, Niger was ranked last in the UNDP 2019 Human Development Index (HDI) and faces rapid population growth.

Climatic shocks are particularly frequent in Niger and have detrimental effects on rural households. Gao and Mills (2018) show how low rainfall and extreme temperature levels have an adverse effect on household consumption, even after taking into account coping strategies. Annan and Sanoh (2018) estimate that household consumption declines by 31-48 percent when exposed to extreme shocks in Niger, which leads to a range of costly coping strategies. Weather shocks were also found to have a negative effect on technology adoption and use of modern inputs (Asfaw et al., 2016) and to be associated with large movements in food prices (Aker et al., 2009). While weather shocks are not necessarily driven by climate change, households in rural Niger report perceiving rainfall as more scare, erratic, often delayed and likely to generate droughts compared to before 2009 (World Bank, 2016). The large welfare costs of shocks highlighted in these studies indicate that households' risk-coping and risk-management strategies are imperfect (see also Asfaw et al., 2018).

Niger has received substantial humanitarian assistance over the years. A range of emergency interventions have been implemented in reaction to shocks and seasonal food insecurity, including cash or food transfers during the lean agricultural season (Aker al. Al, 2016, Hoddinott et al., 2018). Over time, policy makers and development stakeholders have raised questions on how to better articulate emergency responses to cope with realized shocks and the establishment of a more permanent national system to strengthen households' ability to protect themselves against future shocks. In 2011, the Government of Niger initiated a national safety net system anchored in a Safety Net Unit in the Office of the Prime Minister.[7] The objective was to establish multi-year safety nets and develop an adaptive system including shock-responsive interventions. This approach has influenced similar investments in other countries in the region (Bowen et al., 2020).

As part of the national safety net system, a flagship national cash transfer program was put in place. It aimed to improve household food security and consumption, while also facilitating

[7] This was supported by (adaptive) safety net projects which have received 150 million USD funding from the World Bank and the Sahel Adaptive Social Protection Program (with contributions from DFID) through various phases between 2011 and 2025.

investments in economic activities and children's human capital (this latter objective is discussed in Premand and Barry, 2020). The program provides small, regular unconditional transfers of 10,000 CFA (about 20 USD, equal to approximately 15 percent of the poverty line for a rural household) over a period of 24 months. The transfers are targeted to poor households selected based on a proxy-means test, and women are the recipients of the transfer within households.[8] Over time, the program has expanded to reach over 100,000 households in successive phases, or approximatively 1 million individuals.

One of the core rationales for the establishment of this regular cash transfer program, as opposed to short-term transfers during the lean season or in the aftermath of shocks, was that it would also help households better prepare themselves against future shocks. As such, regular transfers would contribute to mitigate some of the need for emergency assistance ex-post. Earlier studies have documented the impacts of a small pilot cash transfer program that preceded the national program on poor households' asset accumulation 18 months after the end of transfers (Stoeffler et al., 2019). In this paper, we focus on analyzing whether cash transfers improve households' resilience through an enhanced ability to manage climatic shocks.

2.2 Study design and data

In 2012, 6 communes in the regions of Dosso and Maradi were selected to benefit from the first phase of implementation of the national cash transfer program. These 6 communes were selected based on geographical targeting and are considered among the poorest in Niger. Given that needs greatly exceeded the project resources, public lotteries were used to select beneficiary villages among all 500 eligible villages in the 6 targeted communes. Prior to the lotteries, small

[8] We discuss the targeting protocols further below. Premand and Schnitzer (2020) discuss the performance of targeting methods in detail.

neighboring villages were grouped into geographical clusters. Public lotteries were organized in the presence of village chiefs, communal authorities and program staff. They led to the selection of 100 clusters (169 villages) to participate in the cash transfer program. A control group of 52 clusters (85 villages) was also drawn for inclusion in the evaluation sample. The lotteries were held separately in each commune, and further stratified between sedentary and nomadic clusters of villages.[9]

After the public lotteries, a detailed listing of households was implemented in all sample villages. A random sample of 30 households was drawn from the listing in each cluster to be included in the baseline survey.[10] The baseline survey was collected between April and June 2012. The sample of 4,330 households included 1,469 households in control villages and 2,861 households in treatment villages. The baseline survey included a questionnaire based on the 2011 Niger LSMS-ISA national household survey, which contained detailed modules on consumption, food security, and economic activities, among other topics.

Following the baseline survey, a registry census collected basic information about all households in treatment villages. The census data were used to calculate a proxy-means test (PMT) score for each household. The households with the lowest PMT scores were selected as beneficiaries for the cash transfer program. On average, 40 percent of households in program areas were selected. Premand and Schnitzer (2020) provide additional details on the implementation of the PMT targeting procedure and analyze targeting efficiency and legitimacy compared to alternative

[9] Additional details on the RCT design are provided in Premand and Barry (2020).

[10] Approximately 20% of households with high self-reported income were excluded from the sampling frame as they were considered ineligible for the cash transfers. The use of such an exclusion filter was shown not to worsen targeting performance (Premand and Schnitzer, 2020).

methods in a subsequent phase of the program (in different communes and using a different experiment).

Importantly, in our case, the registry census to generate PMT scores could not be collected in control villages. For that reason, the baseline survey was designed to collect information to re-calculate PMT scores for each sample household (i.e. both in the treatment and control groups). As such, it was originally planned that the PMT threshold could be re-applied based on the estimated PMT scores calculated from the baseline survey. This would have allowed to make intent-to-treat (ITT) comparisons between actual beneficiaries in the treatment groups and potential beneficiaries identified after applying the same ranking procedure in the control group.

Once data on actual beneficiary status for households in the treatment group became available from the project unit, they were merged with the baseline survey data set. The actual beneficiary status was compared to a predicted beneficiary status re-estimated from the baseline survey for the treatment group. The comparisons highlighted that the prediction of beneficiary status based on the baseline survey was imperfect. Figure 1 illustrates that the PMT score from the registry census accurately predicts beneficiary status. It shows a sharp drop in the probability of being a beneficiary around the eligibility cut-off. This means that the PMT selection procedure was faithfully implemented based on the registry census data.[11] However, the PMT scores re-calculated from the baseline survey predict beneficiary status imperfectly. As can be seen in Figure 1, the likelihood of being "predicted" to become beneficiary based on the baseline survey variables is not as strong a predictor of actual beneficiary status. While the likelihood of being a beneficiary is still strongly associated with the predicted PMT score, the slope of the curve is

[11] In fact, the formula was programmed in SQL as part of the program monitoring and information system, and then separately in Stata. The results were compared to ensure that no programming mistake was made.

flatter, and no sharp drop is observed at the cut-off. This is likely due to measurement errors in household variables that enter the PMT score calculation (possibly in both the PMT and baseline surveys). It may also be due to split-off households between the two surveys.[12] These issues likely reflect teething pains during the establishment of the Niger safety net system, including with the quality of rapid, large-scale data collection for the registry census in a context characterized by low administrative capacity. Indeed, the PMT targeting was shown to be relatively efficient and to suffer from little measurement errors in subsequent phases of the program (Premand and Schnitzer, 2020). In the next section, we discuss the preferred identification strategy given that predicted beneficiary status from the baseline survey imperfectly matches actual beneficiary status in the treatment group.

The cash transfer program was implemented between February 2013 and April 2015. Compliance with cash transfer participation was high: 98% of households selected to receive the cash transfers participated over the course of the program. In addition to cash transfers, a subset of beneficiary villages was randomly selected to receive a behavioral change component to encourage investments in children's human capital. The value-added of this behavioral intervention on early childhood development outcomes is the focus of the original RCT discussed in Premand and Barry (2020). In the present paper, we focus on the pooled cash transfer treatment and its effect on household-level outcomes related to resilience.[13]

[12] Defining household units in the context of large, frequently polygamous households in rural Niger is challenging, and some discrepancies in the application of these definitions may have taken place between surveys. Compared to this type of measurement error, manipulation in household answers is much less likely given the formula was not made public and had not previously been used in study areas.

[13] The analysis of the impact of cash transfers on resilience was not planned when the RCT was designed. However, this question became central both as a research and policy issue during implementation of the Niger safety net project, in particular when the Sahel Adaptive Social Protection Program was set up and provided additional financing to the project with a core objective to improve household resilience.

A follow-up household survey was implemented between mid-January and mid-April 2015. From the original sample, 3,953 households were included in the sample from the follow-up survey, of which 3,803 were tracked at follow-up with complete questionnaires.[14] The attrition rate (3.8 percent) is balanced between treatment and control. The follow-up survey included a household survey questionnaire similar to the baseline survey, with the addition of a few modules on agriculture and other activities.

To complement the survey data, we compiled meteorological satellite data to obtain exogenous measures of climate shocks. Specifically, the follow-up survey collected geo-coordinates for each household, and we calculated the median geo-location for each village. For this analysis, we separate hamlets (*hameau* in French) from villages, because hamlets are related to a village administratively but can be geographically relatively distant. In doing so, we obtain 512 village-hamlet units with unique GPS coordinates. We then merged the household data with satellite data that contained information on climatic variables based on the GPS coordinates of their village-hamlet. The satellite data were obtained from the African Flood and Drought Monitor (AFDM)[15] and include daily rainfall information for the period 1970-2015 at a 0.25 degree resolution. From this information, we constructed the monthly historical average level of rainfall for each of the 512 village-hamlet units for the period that precedes the intervention (1970-2011). We also construct monthly precipitation level variables for the pre-intervention and intervention period (2011-2015).

[14] The follow-up sample was stratified based on the proxy means test score "predicted" from the baseline survey. We dropped from the sample 377 households with high predicted PMT score (hence low probability of being selected as beneficiary) (see Premand and Barry, 2020).
[15] https://platform.princetonclimate.com/PCA_Platform/.

3 Methodology

This section outlines the empirical strategy to estimate the average impacts of cash transfers, as well as to test whether cash transfers improve resilience by enhancing households' ability to mitigate the welfare effects of climatic shocks.

3.1 Specifications to estimate average impacts

As discussed in section 2, a first specification derives intent-to-treat estimates between (potential) beneficiaries in the treatment and control groups. Potential beneficiaries in the control group are identified by applying the same ranking procedure that determined program eligibility in the treatment group, based on an estimated PMT score calculated from the baseline survey and the same PMT cut-off (see section 2.2). In this specification, we include all households whose PMT scores (calculated from the baseline survey) are below the PMT selection threshold, whether they are actual beneficiaries or not. This approach provides a benchmark specification.[16] We apply a Difference-in-Differences (DID) estimator to compare changes in outcomes between 2012 and 2015 for households in the treatment and control groups with baseline predicted PMT score below the PMT selection cut-off ($PMT_{score_i} \leq \ Cutoff$):

$$y_{it} = \beta_0 + \beta_1 \mathrm{T}_i + \beta_2 F_t + \beta_3 \mathrm{T}_i * F_t + \varepsilon_{i,t} \qquad (1)$$

where T_i is an indicator variable for households in treatment villages (with predicted PMT score below the selection cut-off), F_t is an indicator variable equal to 1 at follow-up (in 2015), and β_3

[16] This strategy is preferred to a full-sample ITT specification at the village level. This is because comparisons at the village level, while well-identified given the randomization, would give imprecise estimates of program impacts since only 40 percent of households were treated on average. Among households in the treatment group with baseline PMT scores below the PMT cutoff, 55 percent are actual beneficiaries (see section 2.2 and Figure 1). Households with baseline PMT scores below the PMT cut-off are also very similar across the treatment and control groups (see section 3.2 and Table 1).

captures the intent-to-treat impacts of the cash transfers. Standard errors are clustered at the randomization cluster level (as recommended by Abadie et al., 2017).[17]

Intent-to-treat (ITT) estimates are unbiased but may underestimate the average treatment effects (ATE) of the cash transfers. The predicted beneficiary status imperfectly matches actual beneficiary status since 55% of predicted beneficiary households (based on the baseline PMT score) are actual beneficiaries. In order to estimate the average treatment effects on the treated, our preferred specification leverages the fact that we observe actual beneficiary status in the treatment villages, and that the selection rule was based on clearly established PMT variables (although measured with noise at baseline). We thus estimate a matched difference-in-differences specification (PSM-DID), in which actual beneficiary households in the treatment group are matched with households in the control group based on PMT variables from the baseline survey. Specifically, we implement propensity score matching (Caliendo and Kopeinig, 2008; Rosenbaum and Rubin, 1983) by estimating the propensity to participate to the program $P(X)$ via a probit regression with variables from the PMT formula. Average treatment effects on the treated are then estimated via PSM:

$$\tau^{PSM} = E_{P(X)|T=1}(E[Y(1)|T=1, P(X)] - E[Y(0)|T=0, P(X)]) \qquad (2)$$

where $T = 1$ indicates treatment, $Y(1)$ is the outcome of interest for beneficiary households in the treatment group, and $Y(0)$ for matched households in the control group.[18] In the main specification, we perform one-on-one matching of non-beneficiaries in the control group to

[17] For a limited number of outcomes of interest that are only available at follow-up, we employ a single difference OLS specification. While unbiased in case of random assignment (and since the treatment and control groups are balanced, see section 3.2), this approach is likely to be less precise than the DID specification when baseline information is available (McKenzie, 2012).
[18] The PSM estimator is implemented in Stata as per Leuven and Sianesi (2014).

actual beneficiaries without replacement. We estimate the impact on the "first difference" outcome ($Y_{FD,i} = Y_{2015,i} - Y_{2012,i}$, where 2015 is for follow-up survey and 2012 is for baseline survey) and cluster standard errors at the randomization cluster level, similar to the benchmark DID specification. We also conduct a series of robustness checks by adding different sets of controls and using alternative matching methods.[19]

3.2 Descriptive statistics, shocks, and balance

Table 1 presents baseline descriptive statistics for some key variables in control and treatment villages for the full sample (column 1 and 2), and for the sample of households with predicted (baseline) PMT scores below the program selection threshold (column 4 and 5). Both the full sample and the sample below the (predicted) PMT threshold are very poor, with per capita consumption below 332 CFA per day (0.57 USD). Average livestock herd size is also relatively small (1.58 Tropical Livestock Units, TLU, in the control group). Virtually all households engage in agriculture, with an average land area of 3.75 ha in the control group. Renting or renting out land is rare (0.17 ha and 0.11 ha on average respectively, in the control group). *Tontine* (ROSCA) participation rates are relatively low (12%) but involvement in household enterprises is widespread (68% in control group). Households are large (average of 8.5 members) and household heads have less than one year of education on average.

Column 3 and 6 show the result of balance tests for the respective samples. Overall, there are few significant differences across groups. The treatment and control groups are well balanced, including below the predicted PMT threshold.

[19] For variables that were not collected at baseline, we again run a simple-difference matching specification.

We define drought shocks based on rainfall in June 2014, i.e. at the start of the main planting season the year preceding the follow-up survey. Using the AFDM data, we compute for each village the historical average rainfall in June (for years 1970-2011) as well as the rainfall deficit in June 2014 (the difference between the June 2014 rainfall and the historical average). We define our main shock variable as rainfall below the 20th percentile of the historical distribution. We define shocks similarly for years 2011, 2012 and 2013. Shocks in 2014 (prior to our follow-up survey) are not correlated with shocks the previous years (2011-2013) defined in a similar manner. Shocks are spatially dispersed across and within regions, although more frequent in the Maradi region.[20] 794 households (20.9%) and 111 villages-hamlets (21.7%) are categorized as exposed to a shock during the main agricultural season before the follow-up survey. We also define alternative measures of shocks based on each percentile of rainfall deficits. We focus on the month of June since it is the critical window during the main agricultural season for cereal crops that are most commonly cultivated in Niger. As in other Sahelian regions, rain deficits in the planting season are strongly correlated with crop failures.[21] Table A1 shows that the exogenous shock variable is strongly associated with self-reported measures of drought shocks, but also with agricultural outputs, welfare and food security in the control group. Specifically, drought shocks are associated with a lower production of millet and sorghum by 148 kg, with a lower consumption per capita per year by 17.7%, and a lower food consumption score by 10.4

[20] There is no heterogeneity in our results across region (available upon request), so that regional effects are not driving our results.

[21] For planting season dates, see for instance FAO country briefs for Niger: http://www.fao.org/giews/countrybrief/country.jsp?code=NER. See also Marteau et al. (2011) for the importance of the early rainy season (the month of June) for crop failure in Niger. Our results are consistent with Asfaw et al. (2016) who show that a late onset of the rainy season has a large and significant negative impact on agricultural productivity and income in Niger. For a similar focus on the planting season for identifying exogenous shocks in agriculture in another context, see Macours et al. (2012). Early season rainfall events are often critical for agricultural and livestock systems, and for that reason are often incorporated in the design of index insurance products (Maccini and Yang, 2009; Jensen et al., 2019).

points. Figure A1 shows that the 20th percentile of the historical distribution corresponds to the most severe shocks related to agricultural production and consumption, and correlates well with self-declared shocks.[22] These results are consistent with studies that emphasize the negative effects of low rainfall on agricultural and household outcomes in Niger (Gao and Mills, 2017; Asfaw et al., 2016).

Table A2 documents balance between the treatment and control groups for households in areas exposed to exogenous shocks. There are only minor differences between treatment and control households exposed to shocks, and the two groups are overall well-balanced, including the sample of households below the PMT threshold.

3.3 Measuring resilience

In light of policy makers' increased focus on resilience, several measurement approaches have been proposed in recent years (Constas et al., 2013; Serfilippi and Ramnath, 2018). Barrett and Constas (2014) recommend building specific resilience measures (such as one based on the FGT poverty indices). Cissé and Barrett (2018) follow their conceptual framework to construct a probability-based resilience measure. Upton et al. (2016) apply this approach to food security using a household panel from Northern Kenya. These approaches are consistent with a long-standing literature on vulnerability and poverty dynamics (as noted by Béné et al., 2012 or Constas et al. 2013). However, the measures developed by Cissé and Barrett (2018) are more

[22] Figure A1 shows the coefficient of the regression in Table A1 for various definitions of the shock variable. Specifically, we vary the percentile of the historical distribution used to define the shock variable. For each percentile, we show the regression coefficient for per capita consumption, food consumption score, self-declared shocks and agricultural production.

relevant for analyses based on long panels than for the short panels typically collected for impact evaluations of specific programs.[23]

When precise information on shocks is available, resilience can be proxied more directly by observing the degree to which households can mitigate the effect of shocks on welfare outcomes, along with the types of coping strategies they use (Constas et al., 2013). In the context of impact evaluations, another approach is to analyze how interventions affect households' ability to manage shocks, which can be analyzed by assessing the heterogeneity of program impacts by exposures to climatic shock measures (Macours et al., 2012; Asfaw et al., 2017; Béné et al., 2017; Christian et al., 2019). This is the main approach used in this paper. Specifically, we measure the effect of exogenous climatic shocks on household consumption, and then analyze whether cash transfers mitigate the negative effect of shocks based on an interaction term between shocks and transfers. Exogenous covariate weather shocks are obtained from satellite rainfall data as described in sections 2.2 and 3.2. Similar to (1), the DID specification taking shocks into account can be written as follows, for households with $PMT_{score_i} \leq \ Cutoff$:

$$y_{FD,i,v} = \beta_0 + \beta_1 \mathrm{T}_{i,v} + \beta_2 \mathrm{S}_v + \beta_3 \mathrm{T}_{i,v} * \mathrm{S}_v + \varepsilon_i \tag{3}$$

where $Y_{FD,i,v} = Y_{2015,i,v} - Y_{2012,i,v}$ is the First Difference of the outcome of interest (as in equation (2)) for household i in village-hamlet v, $\mathrm{T}_{i,v}$ is equal to 1 when household i belongs to a village v assigned to cash transfer treatment, and S_v is an indicator variable equal to 1 in case of covariate shock in v at follow-up (2015). Finally, β_3 captures the additional (mitigating) effect of

[23] Measuring resilience in practice is challenging given that the concept evokes long time windows and multiple welfare dimensions (Béné et al., 2016; Béné et al., 2017; see also Carter and Barrett, 2006, for a discussion similar challenges related to measuring poverty dynamics).

the transfers for households affected by shocks.[24] In this specification, β_1 captures the average effect of the transfers, β_2 the effect of shocks, and β_3 households' capacity to mitigate the effect of shocks. We test whether the combined effect $\beta_1 + \beta_3$ is significantly different from 0 to assess whether cash transfers have significant impacts on the subset of households exposed to shocks. We test whether the combined effect $\beta_2 + \beta_3$ is significantly different from 0 to assess whether transfers fully mitigate the effect of shocks. As for equation (1), our preferred specification is a matched-DID estimation that compares actual cash transfer beneficiaries to non-beneficiaries in the control group with similar propensity score (computed from the variables that enter the PMT score calculation; see section 3.1).

As a complementary approach, we apply the methodology from Cissé and Barrett (2018) to construct an indicator that proxies the resilience level of each household. We consider per capita consumption as the main welfare indicator ($W_{i,t}$). We first estimate the resilience level $\hat{\rho}_{i,t}$ of each household i, as the probability to have welfare above a threshold $\overline{W}$ in the next period, given its characteristics $X_{i,t-1}$:

$$\hat{\rho}_{i,t} = P(W_{i,t} \geq \overline{W} | W_{i,t-1}, X_{i,t-1}) \tag{4}$$

To estimate the probability that household i remains above a consumption threshold, we use the control group to estimate the conditional mean and variance of consumption per capita in 2015 as a function of baseline (2012) consumption per capita ($W_{i,2012}$) and other household characteristics ($X_{i,2012}$).[25] Based on this, we then predict the resilience level $\hat{\rho}_{i,2015}$ for each

[24] As such, the specification can be characterized as a triple-difference specification. As explained in section 3.1 for equation (1), we also estimate a single-difference specification for outcomes only available at follow-up.
[25] We use two thresholds: median consumption and the 10th percentile of the consumption distribution (to capture very low levels of consumption). This second threshold is closer to a humanitarian objective. The household characteristics used to predict endline consumption from the baseline survey are the variables used for the PMT

household in our sample, i.e. both in the control and treatment groups. Second, we forecast the resilience level in the next period $\hat{\rho}_{i,future}$ based on current consumption per capita and household characteristics $W_{i,2015}$ and $X_{i,2015}$. This indicates whether changes in characteristics induced by the cash transfer program translate into higher resilience to future shocks. Third, we compute FGT-like resilience measures R_{α} which indicate whether households are resilient or not (resilience headcount ratio, $\alpha = 0$) and their distance to desirable resilience levels (resilience gap, $\alpha = 1$). For this, we define a normative probability $\bar{P}$ which is the resilience threshold,[26] and measure R_{α} as:

$$R_{\alpha,t}\left(\hat{\rho}_{i,t}, \bar{W}, \bar{P}\right) = \left(\frac{\bar{P} - \hat{\rho}_{i,t}}{\bar{P}}\right)^{\propto} \tag{5}$$

Finally, we estimate the impact of the transfers on the resilience level $\hat{\rho}_{i,t}$ and resilience indicators $R_{\alpha,t}$ using the difference-in-differences specification described in equation (1). This provides another way to assess whether households that receive cash transfers are able to improve their resilience.

The Cissé and Barrett (2018) methodology has been used in the context of impact evaluations by Cissé and Ikegami (2016) and Phadera et al. (2019). The methodology relies on several assumptions and predictions. There are caveats to its application in our context with two rounds of data where we do not fully observe welfare dynamics. The estimation of $\hat{\rho}_{i,2015}$ is based on baseline consumption, household characteristics and shocks. This also means that $\hat{\rho}_{i,future}$

formula and a few additional baseline variables (assets, education, household characteristics, commune fixed effects). We follow Cissé and Barrett (2018) and include shock variables to predict endline consumption.

[26] We use a probability $\bar{P} = 0.5$ when $\bar{W}$ is the median consumption. We use $\bar{P} = 0.9$ when $\bar{W}$ is the humanitarian objective of remaining above the 10th consumption percentile. Thus, we measure whether i) households have a moderate likelihood (0.5) to have above-median consumption levels; and ii) they are very likely (likelihood of 0.9) to be above a critical consumption level (10th consumption percentile).

cannot be compared to actual future consumption measures to check the accuracy of the prediction. Lastly, as noted by Upton et al. (2020), the Cissé and Barrett (2018) measure is not automatically a good predictor of actual resilience (although it performs marginally better than alternative RIMA and TANGO methodologies).[27] For all these reasons, and in line with Béné et al. (2017), our preferred identification strategy remains the approach described in equation (3) to assess the mitigating effects of cash transfers on the welfare of households exposed to climatic shocks.

Lastly, we measure how transfers affect consumption dynamics based on follow-up (2015) consumption levels as predicted from baseline. We define a household as "descending" if its consumption per capita is lower in 2015 than in 2012. Next, we use baseline (2012) characteristics to predict follow-up (2015) consumption in the control group. We then categorize households as "predicted to descend" if the 2012 variables predict that their consumption level in 2015 will be lower than in 2012. Finally, we consider a household as "predicted to descend but did not" when its characteristics predicts that consumption will fall ("predicted to descend") but this was not the case (not "descending"). Using OLS and probit models, we measure the impact of receiving transfers on the likelihood to be "descending" and to be "predicted to descend but did not".[28] While this is not a direct measure of resilience, it indicates whether cash transfers prevent households from actual consumption drops, focusing in particular on households whose characteristics indicated that they would fall to lower levels of consumption.

[27] The RIMA methodology is described in FAO (2016) and d'Errico et al. (2018). For an application of the TANGO methodology, see for instance Smith and Frankenberger (2018). Upton et al. (2020) compare these three approaches.
[28] As a placebo test, we also measure whether transfers affect the likelihood to be "predicted to descend" from baseline, which should not be affected because it is based on baseline characteristics only.

The next section presents the average impacts of the transfers from equations (1) and (2), before turning to heterogeneous impacts of the transfers by exposure to climatic shocks as in equations (3) to (5).

4 Results

4.1 Average welfare impacts

Table 2 shows average cash transfer program impacts on consumption and food security, based on the matched-DID specification (equation (2)). Cash transfers improve welfare along several dimensions. They raise consumption per capita by 8,366.6 CFA or 10.4% relative to the control mean at endline. Both food (+5538.5 CFA) and non-food per capita consumption (+2480.5 CFA) increase, leading to a decrease of the poverty gap by 4.3 points (or 9.4 percent relative to the poverty gap in control).[29] On the other hand, the increase in the food consumption score is not significant in this specification, though the coefficient is positive (p-value = 0.111).

Table A3 contains results for the same outcomes based on a DID specification for the sample below the PMT threshold (equation (1)). Results again point to consistent improvements across welfare indicators. Point estimates are very close to those in table 2, though marginal changes in statistical significance are observed. For instance, the impact on per capita food consumption is not significant (+5108 CFA, p-value = 0.145), but the impact on the food consumption score (+4.8 points) is significant. The latter implies a significant decrease in food insecurity by 8.9 percentage points.

[29] To calculate the household poverty gap (Foster et al., 1984), we use a poverty line of 150,755.4 CFA per capita per year, which is the deflated national poverty line (close to 1.9 USD per day in PPP).

Overall, these results show that on average cash transfers increase per capita consumption and alleviate poverty. The average household size is 10.2 in the control group for households below the PMT threshold at baseline, and households receive 120,000 CFA per year (or roughly 12,000 CFA per person per year). Since we find an impact of 8,367 CFA per capita in beneficiary households, these results imply that for each 100 CFA transferred, consumption increases by 70 CFA (of which food consumption increases by 46 CFA). These results are consistent with the meta-analysis by Ralston et al. (2017), who show that on average 1 USD transferred increases consumption by 0.74 USD (of which 0.34 USD for food consumption).

We conduct a series of robustness checks. First, Figure A2 illustrates the quality of matching. There is a wide common support between the treatment and the matched control group, whose propensity scores are very similar (Panel A). This is a consequence of matching on PMT variables, which are observable determinants of eligibility (see sections 2.2 and 3.1). Also, due to the randomization, there are very few differences between the treatment and the control group, and these small differences are further reduced after matching (Panel B). Second, we perform a range of robustness checks of matching specifications. These include (i) two alternative matching methods and (ii) four different sets of household baseline control variables (see Table A4; with additional results available upon request). Results are robust to these alternative specifications, with limited changes in either the estimated coefficients or the significance levels.

In the next section, we disentangle how much of the average welfare gains are driven by impacts when households are exposed to drought shocks. We then analyze the mechanisms through which cash transfers mitigate the welfare effects of shocks, including through savings, assets and income-generating activities.

4.2 Heterogeneity in welfare impacts by exposure to shocks

Table 3 displays results from the matched-DID specification that includes the treatment, shocks and the interaction between treatment and shocks. Consistent with the negative effects of low rainfalls on control group outcomes in Table A1, Table 3 shows large negative effects of shocks on consumption and food security. For instance, households exposed to drought have lower per capita consumption by nearly 21,000 CFA per year (or 0.242 in log). Adverse effects of shocks are observed consistently across all consumption and food security indicators.

Cash transfers have strong positive welfare effects among households affected by drought shocks. For instance, the interaction term between transfer and low rainfall is positive for consumption per capita at approximately 14,092 CFA for the variable in level (column 1) or 0.187 in log (column 2). While the interaction term is borderline not significant for the variable in level (column 1, p-value 0.108), it is significant at 10% for the variable in log (column 2). Most importantly, the cash transfer treatment effects for household exposed to shocks are highly statistically significant in both cases (p-value of 0.013, respectively 0.008). The impact of cash transfers on households exposed to shocks is about 19,000 CFA per capita per year. The magnitude of this effect is noteworthy: it is larger than yearly per capita transfers, pointing to a form of multiplier effect of cash transfers when shocks occur. Section 4.3 will analyze mechanisms and show that households eligible for cash transfers are able to smooth income from agriculture and off-farm businesses. These impacts on income contribute to explain how cash transfers can have welfare effects larger than transfer amounts when shocks occur.

Cash transfers have positive impacts across most welfare dimensions among household exposed to shocks, including food consumption (+ 14,771 CFA, p-value of 0.007), poverty gap (-0.11, p-value of 0.006), food insecurity (-0.157, p-value of 0.093). Estimated effects on non-food consumption and the food consumption score are positive but not significant, though the p-values are relatively close (0.158, respectively 0.166).

Cash transfers contribute to mitigate the negative welfare effects of shocks across welfare dimensions. This is made clear by taking the sum of the shock coefficient and the interaction term. The p-value of a F-test that the sum of the two coefficients is zero is presented at the bottom of table 3. For consumption per capita, we cannot reject the null that shocks have no welfare effects on households eligible for cash transfers (p-values of 0.432 and 0.555). Similarly, the coefficient of the interaction term between cash transfers and shocks is significant for per capita food consumption and the poverty gap. These coefficients are also of similar magnitude that the coefficients of the shock variable, showing that cash transfers fully mitigate the effect of shocks on these welfare dimensions (F-test p-values of 0.703 and 0.578). For the non-food consumption and food security variables, the shock interaction coefficient is not statistically significant, but it still indicates mitigation as we cannot reject the null than the welfare effect of the shock is zero for households eligible for cash transfers (p-value of 0.158, 0.166 and 0.443). As such, we can never reject the null that shocks do not have significant welfare effects on cash transfer beneficiaries. In short, cash transfers effectively protect households' welfare against the adverse effects of shocks.

Figure 2 presents the point estimates obtained from a similar specification with alternative definitions of the shock variable.[30] We define the shock dummy at each percentile of rainfall deficit based on the historical distribution and plot the interaction coefficient between drought and cash transfers (solid red line) as well as the drought coefficient itself (dashed blue line). The

[30] We show results for the log of per capita consumption and for the food consumption score. Figures for other variables are available upon request.

results are robust around the 20th percentile, which is when shocks have the strongest welfare effects (see section 3.3 and Figure A1).

These results have two important implications. First, climatic shocks induce large welfare losses in the study context, but cash transfers fully protect household welfare against shocks. In fact, cash transfers have welfare impacts that are larger than transfer amounts. Second, the average impacts on welfare documented in section 4.1 are almost fully driven by households mitigating the welfare effects of drought shocks. Indeed, the impacts of cash transfers on the consumption and food security outcomes of beneficiaries not affected by shocks are not significant.[31]

So far, we have analyzed the impact of cash transfers on resilience by assessing to what extent cash transfers mitigate the negative welfare effects of shocks. This is in line with the approach used by Macours et al. (2012). To complement this analysis, we also build resilience indicators based on the Cissé-Barrett (2018) methodology (see section 3.3) and estimate program impact on these dimensions. Table 4 presents the results for a range of indicators. Column (1) shows the predicted future consumption based on follow-up (2015) household characteristics. Columns (2) and (3) show the probability that households remain above the median, respectively the 10th percentile of the distribution of per capita consumption in the future. Columns (4) and (5) show the effect of the transfers on the resilience deprivation headcount ratio (R0) using median consumption and the 10th percentile of the consumption distribution as thresholds. Columns (6) and (7) display the resilience deprivation gap (R1). Consistent with findings above using the matched-DID specification, results suggest that cash transfers strengthen households' resilience.

[31] Table A5 shows similar results for the DID specification for the sample below the PMT threshold (equation (1)). Results are highly consistent. We also show in table A6 a matched-DID specification that includes lag shocks in 2011 (the agricultural season prior to the baseline survey). The coefficient of lag shocks is not significant for any of the welfare variables, and the coefficients for 2014-15 are not affected by the introduction of lag shocks.

We find that beneficiary households have a higher probability to be above both the median and the 10th percentile of the consumption distribution. Cash transfers also reduce the resilience deprivation gap. Although this methodology has limitations given we do not have multiple post-intervention survey rounds, these results suggest that transfers led to an improvement in factors that predict higher future per capita consumption (which include current per capita consumption, for which an increase was documented in section 4.1).

Finally, as an alternative, Table A7 shows that transfers reduce the likelihood that households suffer from consumption drops between 2012 and 2015 by 7.7 percentage points (column (1)). For households whose consumption levels are predicted to be lower in 2015 than in 2012 (based on their baseline characteristics, see section 3.3), transfers decrease the likelihood of an actual consumption drop by 4.6 percentage points. Overall, this is another way to show that cash transfers protect households from welfare losses for the subset of households that could be considered vulnerable based on their baseline characteristics.

In this section, we have shown that cash transfers have substantial welfare effects among eligible households exposed to shocks. Findings consistently point to households becoming more resilient in the sense that they can protect themselves against the welfare effects of shocks. The use of recently suggested resilience indicators confirms findings obtained from heterogeneity analysis based on exogenous rainfall data.

4.3 Mechanisms: Savings, income-generating activities, and assets

We now analyze mechanisms that contribute to improve welfare and resilience among households eligible for cash transfers. One key question is whether households' improved ability to mitigate the welfare effects of shocks is solely due to the direct effect of the transfers, or if instead it is due to the way households use the transfers to save, accumulate assets or generate income from economic activities when shocks occur. Section 4.2 has shown that the welfare impacts of cash transfers are larger than the transfer amounts for households exposed to drought shocks. This section discusses the pathways through which cash transfers can have welfare effects larger than program benefits after shocks occur, including by sustaining economic activities and helping households to smooth income.

Savings

Tontines are informal rotating saving and credit groups (similar to ROSCAs) widely used for consumption and investment purposes in rural West Africa where financial markets are thin (Van den Brink and Chavas, 1997; Karlan et al. 2014; Baland et al., 2019). The cash transfer program we study did not systematically promote savings through *tontines* or other instruments.[32] Still, the provision of regular cash transfers may have facilitated households' participation in *tontines*. Table 5 documents impacts on a range of saving variables. Cash transfers increase the likelihood that households participate in a *tontine* by 16.6 percentage points, which is an 80% increase compared to participation in the control group (20.6%). Deposits in *tontines* and amounts withdrawn increase as well. Shocks do not affect participation or deposits significantly, and cash transfers do not affect the amounts saved when shocks occur either. Overall, cash transfers increase participation in savings groups and amounts saved irrespective of exposure to shocks.[33]

Agriculture and off-farm activities

[32] The program we study in this paper contrasts with an earlier cash transfer pilot in Niger which actively encouraged households to create *tontines* and had sustained effects on assets (Stoeffler et al., 2019).
[33] These results are consistent with effects of cash transfers on saving found in the literature (Aker, 2017; Evans et al., 2014; Gilligan et al., 2009; Ward et al., 2010).

Table 6 documents impacts of cash transfers on agricultural activities at follow-up. This is based on a single-difference specification (see section 3). Agriculture is by far the main occupation in the study sample, with nearly all households engaged in cultivating land. In absence of shocks, cash transfers do not have significant impacts on the use of inputs (fertilizers, column 1), on the area cultivated or yields for the main crop (millet/sorghum, columns 2-3), or on the likelihood that households sell part of the harvest or the value of these sales (columns 4-5). Drought shocks, however, tend to reduce yields by 34.4kg/ha (marginally insignificant, but note this is a noisy measure). Shocks also reduce the likelihood that households sell products to the market and the value of these sales.

When drought shocks occur, cash transfers contribute to an intensification of production through a substantial increase in the number of fields in which fertilizers are applied (+0.157, a 42% increase relative to control). We also find that cash transfers allow households exposed to shocks to keep selling part of their production and to sustain revenues from these harvest sales. The interaction effect between cash transfers and shocks for the value of crop sales is significant and of large magnitude (+15,287 CFA), an increase of 57% relative to control (+13.4 percentage points or 29% for the likelihood of selling part of the harvest). Cash transfers mitigate the reduction of the value of crop sales induced by drought shocks (-26,420 CFA), so that we cannot reject the null that crop sales are unaffected by shocks (p-value=0.167). While we do not observe a significant effect on yields, the interaction term is positive, and yield estimates are notoriously noisy. As such, the positive impacts on harvest sales is suggestive of an increase in the total value of agricultural production (considering the full portfolio of crops). Overall, these results highlight a first channel through which cash transfers facilitate income smoothing: households eligible for cash transfers hit by shocks early in the crop cycle intensify production, and as such are able to maintain revenues from crop sales.[34] Another potential pathway could be that cash transfers may have allowed beneficiary households to sell their harvest later in the season when prices are higher (as in Delavallade and Godlonton, 2020), though we do not have direct evidence for that channel.

Off-farm household enterprises constitute a second mechanism through which cash transfers can facilitate income smoothing when shocks occur. As for agriculture, results on household enterprises reveal heterogeneity in impact by exposure to climatic shocks (Table 7). On the one hand, cash transfers do not affect the likelihood of operating an off-farm household enterprise or the number of household enterprises operated by households not affected by shocks. On the other hand, drought shocks reduce the likelihood that households operate off-farm enterprises and profits from these enterprises. Interestingly, when climatic shocks occur, cash transfer beneficiaries are more likely to keep operating a household enterprise (by 14.8 percentage points, a 27% increase relative to control). They operate on average 0.30 more household enterprises (from 0.73 in the control group). These effects are driven by small businesses related to food processing, cooking and selling.

Table 8 documents impacts on profits in household enterprises. This is based on a single-difference specification (see section 3) since the outcomes are only observed at follow-up. In absence of shocks, cash transfers do not increase yearly profits in household enterprises. However, cash transfers increase profits by a substantial magnitude among households exposed

[34] Table A8 documents cash transfer program impacts on land ownership and usage (matched-DID specification, equation 2). There is no evidence of impacts on this dimension, which is not surprising given frictions in land markets in Niger. The areas rented, rented out, the number of fields or the likelihood of small infrastructure investment do not seem to be affected by cash transfers, low rainfall, or the interaction between cash transfers and drought. Cash transfer beneficiaries may cultivate slightly less land when hit by a shock, which is consistent with an intensification of farming activities on the areas that are cultivated, while low-return plots affected by shocks may be abandoned.

to shocks. The magnitude of the interaction coefficient for yearly profits is 61,214 CFA (or 7,452.7 CFA for profits during the last month), which fully offsets the negative effect of shocks on profits (-51,062.9 CFA for yearly profits, -7,493.1 for profits during the last month). Similarly to crop sales, the cash transfer program thus also facilitates income smoothing by helping households sustain revenues and profits from off-farm enterprises when shocks occur. This partly explains how cash transfers have welfare impacts larger than the transfer amounts among households affected by shocks.

There are various mechanisms through which the operation of off-farm household enterprises can facilitate income smoothing. Table 8 (column 4) shows that cash transfer beneficiary households are more likely to finance household enterprises from agriculture when exposed to drought shocks (column 4). The existence of interlinkages between agriculture and off-farm enterprises is further illustrated by impacts on participation in household enterprises being driven by activities involving the processing of agricultural products (Table 7, column 3). As such, the fact that cash transfer beneficiaries sustain crop sales may be critical for them to generate liquidity to purchase inputs for household enterprises. Similarly, if cash transfers help sustain agricultural production or sales, they could also ensure the direct provision of products as inputs into household enterprises. This would be another mechanism explaining how income smoothing is achieved in household enterprises. Cash transfer beneficiaries could also smooth income by selling products to neighboring communities less hit by shock (as in Macours et al., 2012), though we do not have evidence on this channel in the study context. Lastly, we cannot rule out demand-side effects, whereby the influx of cash in communities sustains demand for products (as highlighted by increases in consumption highlighted in section 4.2) when communities are hit by a shock. This channel appears less likely, however, because cash transfers only target 40% of households in participating communities, a relatively low saturation level compared to cash transfer programs elsewhere.[35]

Assets

Cash transfer impacts on assets are more limited. There is no effect on the total value of assets in absence of shocks (Table 9). Shocks have a negative effect on the overall value of assets, and this effect is mitigated by the cash transfers, suggesting that households smooth assets (as in Zimmerman and Carter, 2003; and Carter and Lybbert, 2012).[36] We do not find impacts on housing variables either (type of roof, etc., available upon request), though these are not expected to change over the duration of a short program.

Table 10 documents impacts on livestock. There is no impact on an overall livestock index.[37] The only significant impacts are an increase in the likelihood to own goats or sheep, though it is compensated by a decrease in the likelihood of holding poultry. Shocks do not affect livestock holding, which is consistent with several studies showing that livestock is not widely used as a buffer to shocks in the Sahel (Fafchamps et al., 1998; Kazianga and Udry, 2006; Carter and Lybbert, 2012). Not surprisingly given that shocks do not affect livestock, the interaction between cash transfers and shocks is not significant, except for a small positive effect on having poultry.

5 Discussion and conclusion

[35] Premand and Barry (2020) do not find that the Niger cash transfer program affects product prices.
[36] The earlier evaluation of a Niger cash transfer pilot did not find significant impacts on durables 18 months after project termination either (Stoeffler et al., 2019).
[37] This contrasts which a significant increase 18 months observed after the end of the transfers in the impact evaluation of an earlier cash transfer pilot in Niger (Stoeffler et al., 2019).

Do cash transfers enhance households' resilience by protecting them from the welfare effects of shocks? In this paper, we answer this question by analyzing data from the randomized evaluation of a government-led, multi-year cash transfer program in rural Niger combined with satellite data on exogenous weather shocks. The setting is one of the poorest in the world and a particularly shock-prone environment. We document average program impacts on a wide range of outcomes. We then analyze heterogeneity in impacts by exposure to drought shocks. By doing so, we estimate the effect of cash transfers in the absence of shocks, as well as the mitigating effects of cash transfers when shocks occur. We also analyze the strategies that households employ to manage shocks, as such documenting the mechanisms though which small, regular transfers contribute to building resilience.

This study provides evidence that cash transfers can help households mitigate the adverse effects of climatic shocks. Transfers raise average per capita consumption by about 10 percent on average. This effect is mostly driven by households affected by droughts, for whom welfare impacts are larger than the transfer amount and fully offset losses induced by shocks. Turning to the mechanisms, beneficiaries have higher participation and savings in *tontines*. Importantly, we show that cash transfers improve household capacity to smooth income when shocks occur. We observe an intensification of agricultural activities, with increased revenues from sales of agricultural products. The likelihood that households operate an off-farm enterprise and related profits are also higher when shocks occur. We find more limited impacts on assets and livestock. Overall, these results suggest that households benefit from investing the transfers in income-generating activities. This explains how the magnitude of the impacts on consumption is larger than the value of the transfers among households hit by a shock. The findings are consistent with safety nets enhancing resilience.

Could the same results be achieved by simply rolling out emergency transfers after shocks occur? In principle, a better ability to deal with shocks can stem either from the direct effect of the transfers, or from an improved ability to save or generate revenues when shocks occur. For instance, income smoothing may be achieved either by intensifying agricultural production or diversifying income sources outside agriculture. In the study setting, an enhanced ability to smooth income appears as an important channel to improve resilience. In particular, it explains why cash transfers have welfare effects larger than transfer amounts among households exposed to shocks. The presence of a multi-year cash transfer program in place before the realization of shocks thus appears instrumental in fostering resilience in the context of our study. Given the critical importance of timing for the agricultural activities and related household enterprises on which we observe impacts, the existence of a multi-year program providing predictable transfers is likely to be more effective than emergency relief in supporting livelihoods. Still, the analysis of the effectiveness of shock-responsive transfer programs rolled out based on shock triggers in supporting household welfare, income smoothing and livelihoods would deserve additional research.

This paper makes several contributions to the literature. It provides a better understanding of the mechanisms through which cash transfers improve resilience, which relates to one of the original safety net objectives of "protecting households from shocks". It links the two core components of anti-poverty interventions described by Ravallion in his history of poverty, namely "protecting" households and "promoting" their productive capacities (Ravallion et al., 1995; Ravallion, 2015). Our findings are important for the Sahel and for the Africa region more generally, given current efforts to better articulate humanitarian and development interventions in a context of recurring shocks, high poverty rates and limited resources. Also, our results are obtained in the context of a safety net program led by the Government of Niger that has reached approximatively 1 million individuals. As such, they highlight the potential of scaling up such approaches in low-income countries. While some of the productive impacts observed are likely to increase the capability of households to generate revenues after they stop receiving transfers, we only measure impacts at the end of the intervention. As such, we cannot answer questions about the sustainability of these impacts on resilience. Ongoing research is underway in the Sahel to test how best to enhance safety nets with additional productive interventions to maximize program impacts on welfare and resilience, or on the effectiveness of integrated resilience programs such as those implemented by humanitarian organizations.

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Table 1: Test of baseline balance

	(1) Control group	(2) Treatment group	(3) T-test p-value	(4) Predicted beneficiary, Control	(5) Predicted beneficiary, Treatment	(6) T-test p-value
Household Size	8.35	8.61	1.16	10.2	10.2	-0.26
Nomadic community	0.092	0.15	1.27	0.10	0.12	0.43
Number of years of education	0.55	0.36	-2.62***	0.41	0.30	-1.28
Low quality dwelling walls (stone, wood, straw)	0.11	0.12	0.23	0.13	0.14	0.16
Metal roof	0.052	0.054	0.16	0.045	0.040	-0.32
Household head has a handicap	0.032	0.024	-1.45	0.052	0.038	-1.19
Number of shocks last year (self-declared)	2.37	2.40	0.36	2.35	2.39	0.44
Consumption per capita (CFA/pyear)	121062.9	114413.0	-1.47	96928.1	93352.0	-0.89
Food consumption score (0-112, 0 = low)	50.3	49.2	-0.63	48.4	46.0	-1.25
A household member participates in a tontine	0.12	0.12	0.051	0.10	0.11	0.39
Tontine last deposit (CFA)	115.8	105.1	-0.17	52.3	113.1	1.85*
Log value of durables	10.9	10.8	-0.76	10.7	10.6	-0.97
Has a household enterprise	0.68	0.66	-1.00	0.66	0.64	-0.79
Number of household enterprises	0.94	0.90	-0.95	0.92	0.88	-0.62
Household owns a bovine	0.50	0.51	0.15	0.46	0.42	-1.04
Household owns a sheep or goat	0.88	0.87	-1.03	0.85	0.83	-0.93
Household owns poultry	0.57	0.61	1.61	0.50	0.56	1.73*
TLU (99%)	1.58	1.68	0.67	1.44	1.30	-0.83
Cultivates land	0.99	0.99	-1.24	0.99	0.99	-0.41
Land area cultivated (ha)	3.75	3.86	0.55	3.60	4.01	1.53
Number of fields	2.48	2.42	-0.72	2.42	2.44	0.27
Area rented (ha)	0.17	0.13	-1.24	0.091	0.13	1.25
Area rented out (ha)	0.11	0.094	-0.55	0.063	0.092	1.01
Built something against erosion	0.11	0.099	-0.49	0.10	0.11	0.39
Observations	1266	2537	3803	560	1122	1682

Test of balance: group comparisons, full sample (columns 1-3) and below the (baseline) PMT threshold sample (columns 4-6). Standard errors are clustered at the village level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 2: Consumption and Food security, PSM-DID estimation, average effects

	(1) Consumption per capita (CFA/year)	(2) Log consumption per capita	(3) Food consumption per capita	(4) Non-food consumption per capita	(5) Household poverty gap	(6) Food consumption score	(7) Moderate or severe food insecurity
CT beneficiary,	8366.6**	0.0945**	5538.5*	2480.5*	-0.0428**	3.308	-0.0651
2015	(2.03)	(2.10)	(1.80)	(1.78)	(-2.05)	(1.60)	(-1.53)
Observations	2192	2192	2192	2192	2192	2192	2192
Mean in control	81975.000	11.314	60980.113	17767.279	0.456	41.500	0.000
Median in control	92855.320	11.289	69436.243	22721.272	0.418	42.423	0.408

t statistics in parentheses

Estimation of a model of propensity score matching combined with difference-in-difference. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 3: Consumption and Food security, PSM-DID specification, heterogeneity by exposure to shocks

	(1) Consumption per capita (CFA/year)	(2) Log consumption per capita	(3) Food consumption per capita	(4) Non-food consumption per capita	(5) Poverty gap	(6) Food consumption score	(7) Moderate or severe food insecurity
Cash Transfers (CT), 2015	4913.7	0.0495	2673.6	1871.1	-0.0211	1.909	-0.0352
	(1.05)	(0.99)	(0.77)	(1.16)	(-0.90)	(0.86)	(-0.71)
Drought in 2014 (p20) (shock)	-20756.8**	-0.242**	-14283.3**	-6243.6**	0.115**	-11.84***	0.192*
	(-2.32)	(-2.52)	(-2.11)	(-2.13)	(2.58)	(-2.62)	(1.86)
Cash Transfers, 2015 * drought in 2014 (p20)	14092.0	0.187*	12097.4*	2131.4	-0.0906**	5.345	-0.122
	(1.62)	(1.91)	(1.95)	(0.65)	(-1.98)	(0.98)	(-1.12)
Observations	2192	2192	2192	2192	2192	2192	2192
Mean in control	92855.320	11.289	69436.243	22721.272	0.418	42.423	0.408
Median in control	81975.000	11.314	60980.113	17767.279	0.456	41.500	0.000
CT+CT*shock=0	0.013	0.008	0.007	0.158	0.006	0.141	0.093
shock+CT*shock=0	0.432	0.555	0.703	0.158	0.578	0.166	0.443

t statistics in parentheses

Estimation of a matched difference-in-differences model. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Shocks are defined as rainfall in June 2014 below the 20th percentile of the historical average. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 4: Impact on resilience indicators, Cisse-Barrett approach

	(1) Future predicted consumption (FD)	(2) Probability to be above median per capita consumption (FD)	(3) Probability to be above 10p per capita consumption (FD)	(4) R0 indicator, above median (FD)	(5) R0 indicator, above 10p (FD)	(6) R1 indicator, above median (FD)	(7) R1 indicator, above 10p (FD)
Village received cash	2593.7**	0.0179**	0.0235*	-0.00976	-0.00812	-0.0254**	-0.0250*
	(2.09)	(1.99)	(1.91)	(-0.55)	(-0.37)	(-2.03)	(-1.91)
Observations	1515	1515	1515	1515	1515	1515	1515

t statistics in parentheses

OLS estimation of a simple difference model comparing treatment and control villages at endline. Sample of households with estimated baseline PMT score below selection threshold. Estimation based on the Cisse-Barrett approach: see text for details. Future predicted consumption is per capita consumption predicted based on follow-up (2015) household characteristics. Based on these values and on predicted variance, the probability to remain above the median (respectively 10th percentile) of the per capita consumption distribution is estimated. A resilience deprivation headcount (R0) and a resilience deprivation gap R1 are computed using probability thresholds of 0.5, respectively 0.9 for the median respectively the 10th percentile of the consumption distribution. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 5: Savings and Tontines, PSM-DID specification, heterogeneity by exposure to shocks

	(1) HH participates in tontine	(2) Number tontine (6 max)	(3) Tontine last deposit (CFA)	(4) Tontine last amount received (CFA, 99p)
Cash Transfers (CT), 2015	0.166***	0.156***	276.1***	1903.3***
	(4.60)	(3.26)	(4.17)	(2.94)
Drought in 2014 (p20) (shock)	-0.0544	-0.0973	5.934	-552.4
	(-1.05)	(-1.35)	(0.08)	(-0.63)
Cash Transfers, 2015 * drought in 2014 (p20)	-0.0585	-0.0915	-86.15	-784.1
	(-0.76)	(-0.81)	(-0.65)	(-0.45)
Observations	2190	2192	2192	2192
Mean in control	0.206	0.271	153.009	1399.068
Median in control	0.000	0.000	0.000	0.000
CT+CT*shock=0	0.132	0.549	0.100	0.505
shock+CT*shock=0	0.170	0.107	0.553	0.474

t statistics in parentheses

Estimation of a matched difference-in-differences model. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Shocks are defined as rainfall in June 2014 below the 20th percentile of the historical average. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 6: Agriculture, ITT-SD estimation, heterogeneity to shocks

	(1) Fields with fertilizer (#)	(2) Millet / sorghum area (ha)	(3) Millet / sorghum yields (kg/ha)	(4) Sold part of harvest	(5) Value of harvest sold (CFA)
Village received cash	-0.0222	0.0711	-2.722	-0.00878	-1274.2
	(-0.41)	(0.31)	(-0.22)	(-0.23)	(-0.28)
Drought in 2014 (p20)	-0.0844	0.312	-34.40	-0.237^{***}	-26419.7^{***}
	(-0.93)	(1.07)	(-1.65)	(-3.61)	(-4.02)
Village received cash, 2015 when drought in 2014 (p20)	0.157^{*}	-0.0104	7.503	0.134^{*}	15287.2^{**}
	(1.90)	(-0.03)	(0.32)	(1.75)	(2.32)
Observations	1682	1682	1682	1682	1682
Mean in control	0.37	3.08	222.46	0.47	26679.29
Median in control	0	3	190	0	0
CT+CT*shock=0	0.028	0.829	0.809	0.067	0.005
shock+CT*shock=0	0.448	0.294	0.155	0.169	0.167

t statistics in parentheses

OLS estimation of a simple difference model comparing treatment and control villages at endline. Sample of households with estimated baseline PMT score below selection threshold. Shocks are defined as June 2014 rainfall difference with historical average below difference 20th percentile. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 7: Household enterprises, PSM-DID specification, heterogeneity by exposure to shocks

	(1) Has a household enterprise	(2) Number of household enterprises	(3) Has a HHE related to processing agricultural products	(4) Has a HHE (other types)
Cash Transfers (CT), 2015	-0.00590	-0.0572	-0.00738	-0.0210
	(-0.17)	(-0.97)	(-0.25)	(-0.52)
Drought in 2014 (p20) (shock)	-0.198***	-0.368***	-0.274***	-0.0721
	(-2.92)	(-3.33)	(-5.97)	(-0.95)
Cash Transfers, 2015 * drought in 2014 (p20)	0.148*	0.297**	0.232***	0.0592
	(1.87)	(2.13)	(3.94)	(0.63)
Observations	2192	2192	2192	2192
Mean in control	0.554	0.727	0.271	0.401
Median in control	1.000	1.000	0.000	0.000
CT+CT*shock=0	0.048	0.063	0.000	0.657
shock+CT*shock=0	0.430	0.551	0.446	0.858

t statistics in parentheses

Estimation of a matched difference-in-differences model. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Shocks are defined as rainfall in June 2014 below the 20^{th} percentile of the historical average. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1^{st} and 2^{nd} row, respectively 2^{nd} and 3^{rd} row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Household enterprise profits and revenues, ITT-SD estimation, heterogeneity by exposure to shocks

	(1) Yearly profits, all HHE (CFA, 99p)	(2) Profits in last month active, all HHE (CFA, 99p)	(3) Log value HHE equipment (CFA)	(4) HHE financed by agriculture	(5) HH financed by saving	(6) HHE financed by loan	(7) HHE financed by tontine
Village received cash	-23962.3	-1892.2	-0.220	-0.0451	0.0118	-0.0259	-0.00488
	(-1.08)	(-0.70)	(-0.79)	(-1.53)	(0.73)	(-1.12)	(-0.77)
Drought in 2014 (p20)	-51062.9*	-7493.1**	0.0818	-0.101*	0.0897**	-0.0120	-0.0137
	(-1.97)	(-2.15)	(0.12)	(-1.96)	(2.54)	(-0.25)	(-1.49)
Village received cash, 2015 when drought in 2014 (p20)	61214.2**	7452.7**	0.579	0.147**	-0.0797**	0.0646	0.0290*
	(2.17)	(2.15)	(0.90)	(2.50)	(-2.05)	(1.27)	(1.89)
Observations	1682	1682	1682	1682	1682	1682	1682
Mean in control	121927.86	15087.32	3.55	0.30	0.10	0.12	0.01
Median in control	18000	4000	0	0	0	0	0
CT+CT*shock=0	0.037	0.009	0.528	0.042	0.060	0.388	0.060
shock+CT*shock=0	0.705	0.990	0.230	0.374	0.779	0.301	0.438

t statistics in parentheses

OLS estimation of a simple difference model comparing treatment and control villages at endline. Sample of households with estimated baseline PMT score below selection threshold. Shocks are defined as June 2014 rainfall difference with historical average below difference 20th percentile. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 9: Household assets, PSM-DID specification, heterogeneity by exposure to shocks

	(1) Log value of assets (all types)	(2) Log value of furniture	(3) Log value of all household durables	(4) Log value of farm assets
Cash Transfers, 2015	0.0436	0.117	0.0442	0.138
	(0.41)	(0.42)	(0.14)	(0.84)
Drought in 2014 (p20)	-0.413**	-0.757	-0.455	-0.0307
	(-2.33)	(-1.63)	(-0.99)	(-0.10)
Cash Transfers, 2015 * drought in 2014 (p20)	0.539**	0.749	0.335	0.396
	(2.22)	(1.31)	(0.50)	(1.18)
Observations	2180	2192	2192	2192
Mean in control	10.374	8.266	5.934	8.743
Median in control	10.700	9.741	8.006	9.059
CT+CT*shock=0	0.010	0.089	0.525	0.073
shock+CT*shock=0	0.582	0.988	0.812	0.274

t statistics in parentheses

Estimation of a matched difference-in-differences model. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Shocks are defined as rainfall in June 2014 below the 20th percentile of the historical average. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level. Asset value is resale value. All types (column 1) include columns 2, 3 and 4. Furniture durables include sofas, tables, etc. Household durables include bicycles, cellphones, etc. Farm assets include ploughs, carts, etc.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 10: Livestock, PSM-DID estimation, heterogeneity by exposure to shocks

	(1) Household owns a bovine	(2) Household owns a sheep or goat	(3) Household owns poultry	(4) TLU (99%)
Cash Transfers, 2015	0.0226 (0.70)	0.0560^{***} (2.90)	-0.108^{***} (-2.88)	0.0237 (0.22)
Drought in 2014 (p20)	-0.0211 (-0.29)	0.0211 (0.56)	-0.00743 (-0.10)	-0.0274 (-0.21)
Cash Transfers, 2015 * drought in 2014 (p20)	-0.0883 (-1.20)	-0.00341 (-0.09)	0.198^{**} (2.28)	-0.0262 (-0.15)
Observations	2192	2192	2192	2157
Mean in control	0.509	0.885	0.583	1.508
Median in control	1.000	1.000	1.000	1.000
CT+CT*shock=0	0.325	0.110	0.253	0.984
shock+CT*shock=0	0.071	0.676	0.012	0.720

t statistics in parentheses

Estimation of a matched difference-in-differences model. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Shocks are defined as rainfall in June 2014 below the 20th percentile of the historical average. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A1: Effects of exogenous shocks in control group

	(1) Millet / sorghum production (kg)	(2) Difference in log per capita consumption from 2012 to 2015	(3) Difference in Food consumption score from 2012 to 2015	(4) Self-reported drought
Drought in 2014	-147.8*	-0.177*	-10.43**	0.276***
(p20)	(-1.94)	(-1.67)	(-2.29)	(2.95)
N	1266	1266	1263	1266
Mean in control	637.47	-0.28	-7.80	0.31
Median in control	450	-0	-8	0

t statistics in parentheses
OLS estimation. Sample of control households at follow-up. Shocks are defined as June 2014 rainfall difference with historical average below difference 20th percentile. Commune fixed-effects are included. Standard errors are clustered at the village level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A2: Test of baseline balance, households with a shock

	(1) Control group	(2) Treatment group	(3) T-test p-value	(4) Predicted beneficiary, Control	(5) Predicted beneficiary, Treatment	(6) T-test p-value
Household Size	9.09	9.06	-0.13	10.6	10.5	-0.092
Nomadic community	0	0.071	2.03**	0	0.0095	1.34
Number of years of education	0.60	0.65	0.33	0.56	0.47	-0.41
Low quality dwelling walls (stone, wood, straw)	0.091	0.12	0.57	0.083	0.14	1.05
Metal roof	0.028	0.043	0.55	0.035	0.014	-0.60
Household head has a handicap	0.038	0.026	-0.80	0.069	0.038	-1.00
Number of shocks last year (self-declared)	2.19	2.30	0.69	2.22	2.26	0.31
Consumption per capita (CFA/year)	113622.0	105838.5	-0.94	92359.6	78102.5	-1.89*
Food consumption score (0-112, 0 = low)	50.9	50.6	-0.099	46.2	45.5	-0.18
A household member participates in a tontine	0.077	0.15	2.25**	0.069	0.12	1.59
Tontine last deposit (CFA)	40.7	69.7	1.17	36.8	78.2	1.15
Log value of durables	10.7	10.5	-0.80	10.4	10.1	-1.74*
Has a household enterprise	0.72	0.65	-1.53	0.69	0.65	-0.60
Number of household enterprises	1.02	0.88	-1.52	0.94	0.88	-0.60
Household owns a bovine	0.43	0.48	0.99	0.37	0.37	0.029
Household owns a sheep or goat	0.88	0.89	0.22	0.82	0.84	0.50
Household owns poultry	0.54	0.55	0.15	0.40	0.41	0.15
TLU (99%)	1.34	1.70	1.39	1.03	0.99	-0.32
Cultivates land	1.00	0.99	-0.52	0.99	1	1.05
Land area cultivated (ha)	3.13	3.85	2.30**	2.79	3.77	3.02***
Number of fields	2.50	2.54	0.22	2.37	2.49	0.58
Area rented (ha)	0.098	0.080	-0.64	0.057	0.079	0.62
Area rented out (ha)	0.046	0.075	1.21	0.016	0.079	2.04**
Built something against erosion	0.080	0.096	0.43	0.097	0.12	0.44
Observations	286	508	794	144	211	355

Test of balance: group comparisons, full sample affected by shocks (columns 1-3) and below the (baseline) PMT threshold sample affected by shocks (column 4-6). Shocks are defined as June 2014 rainfall difference with historical average below difference 20th percentile.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A3: Consumption, DID estimation, below PMT threshold

	(1) Consumption per capita (CFA/year)	(2) Log consumption per capita	(3) Food consumption per capita	(4) Non-food consumption per capita	(5) Household poverty gap	(6) Food consumption score	(7) Moderate or severe food insecurity
Village received cash, 2015	8999.2*	0.109*	5108.0	2907.9*	-0.0469*	4.834**	-0.0890*
	(1.89)	(1.93)	(1.46)	(1.96)	(-1.81)	(2.13)	(-1.78)
2015	-18718.7***	-0.233***	-22097.4***	3838.4***	0.104***	-5.995***	0.0827**
	(-4.97)	(-5.49)	(-8.04)	(3.24)	(5.14)	(-3.39)	(2.07)
Village received cash	-5492.5	-0.0602	-3604.6	-1395.5	0.0291	-2.213	0.0388
	(-1.44)	(-1.42)	(-1.24)	(-1.31)	(1.44)	(-1.20)	(1.06)
Observations	1682	1682	1682	1682	1682	1680	1680
Mean in control	87568.74	11.23	69720.18	17247.26	0.45	45.45	0.36
Median in control	78791	11	61232	13349	0	45	0

t statistics in parentheses
OLS estimation, sample of households below the (baseline) PMT threshold and surveyed at baseline. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A4: Consumption and Food security, PSM-DID estimation, average effects, robustness to inclusion of control variables

	(1) Consumption per capita (CFA/year)	(2) Log consumption per capita	(3) Food consumption per capita	(4) Non-food consumption per capita	(5) Household poverty gap	(6) Food consumption score	(7) Moderate or severe food insecurity
CT beneficiary,	7791.6*	0.0935**	4928.3	2456.2*	-0.0424**	3.852*	-0.0765*
2015	(1.89)	(2.12)	(1.58)	(1.86)	(-2.06)	(1.92)	(-1.86)
Observations	2192	2192	2192	2192	2192	2190	2190
Mean in control	92855.320	11.289	69436.243	22721.272	0.418	42.423	0.408
Median in control	81975.000	11.314	60980.113	17767.279	0.456	41.500	0.000

t statistics in parentheses

Estimation of a model of propensity score matching combined with difference-in-difference. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level. Control variables are the PMT variables used for the matching algorithm.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A5: Consumption, DID estimation, below PMT threshold, heterogeneity by exposure to shocks

	(1) Consumption per capita (CFA/year)	(2) Log consumption per capita	(3) Food consumption per capita	(4) Non-food consumption per capita	(5) Household poverty gap	(6) Food consumption score	(7) Moderate or severe food insecurity
Village received cash	1393.2	0.0211	-456.9	980.2	-0.00501	3.200	-0.0537
	(0.27)	(0.34)	(-0.12)	(0.61)	(-0.18)	(1.26)	(-0.98)
Drought in 2014 (p20)	-21303.6**	-0.271**	-14119.3*	-6809.9**	0.124**	-10.36**	0.232*
	(-2.28)	(-2.25)	(-1.96)	(-2.45)	(2.33)	(-2.09)	(1.93)
Village received cash, 2015 when drought in 2014 (p20)	22655.1***	0.272**	17086.9***	5146.0*	-0.127***	4.967	-0.121
	(2.77)	(2.50)	(2.73)	(1.85)	(-2.72)	(0.91)	(-1.05)
Observations	1682	1682	1682	1682	1682	1680	1680
Mean in control	96928.07	11.35	80768.87	15328.04	0.39	48.45	0.32
Median in control	88670	11	73994	12147	0	49	0
CT+CT*shock=0	0.001	0.002	0.002	0.009	0.001	0.088	0.075
shock+CT*shock=0	0.882	0.999	0.648	0.538	0.951	0.236	0.228

t statistics in parentheses

OLS estimation, sample of households below the (baseline) PMT threshold and surveyed at baseline. Shocks are defined as rainfall in June 2014 below the 20th percentile of the historical average. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A6: Consumption and Food security, PSM-DID estimation, heterogeneity by exposure to shocks, 2013 lags

	Consumption per capita (CFA/year)	Log consumption per capita	Food consumption per capita	Non-food consumption per capita	Household poverty gap	Food consumption score (0-112, 0 = low)	Moderate food insecurity (=1)
Cash Transfers (CT), 2015	2713.1	0.0255	922.8	1423.4	-0.00883	0.614	-0.0178
	(0.50)	(0.45)	(0.23)	(0.77)	(-0.33)	(0.24)	(-0.33)
Drought in 2014 (p20) (shock)	-21889.4**	-0.255***	-15184.3**	-6474.0**	0.121***	-12.51***	0.201*
	(-2.41)	(-2.61)	(-2.20)	(-2.17)	(2.68)	(-2.73)	(1.93)
Cash Transfers, 2015 * drought in 2014 (p20)	16288.8*	0.211**	13845.2**	2578.3	-0.103**	6.638	-0.139
	(1.80)	(2.09)	(2.16)	(0.76)	(-2.19)	(1.19)	(-1.25)
Drought in 2013 (p20) (shock)	-8255.8	-0.0334	-7779.1	593.9	0.0249	-4.950	0.0597
	(-0.82)	(-0.33)	(-1.01)	(0.16)	(0.55)	(-1.06)	(0.53)
Cash Transfers, 2015 * drought in 2013 (p20)	11044.2	0.108	9049.1	1754.1	-0.0569	6.318	-0.0836
	(1.01)	(0.94)	(1.06)	(0.48)	(-1.00)	(1.38)	(-0.74)
Observations	2192	2192	2192	2192	2192	2192	2192
Mean in control	92855.320	11.289	69436.243	22721.272	0.418	42.423	0.408
Median in control	81975.000	11.314	60980.113	17767.279	0.456	41.500	0.000
CT+CT*shock=0	0.013	0.008	0.007	0.158	0.006	0.142	0.093
shock+CT*shock=0	0.513	0.645	0.817	0.185	0.678	0.216	0.504

t statistics in parentheses

Estimation of a matched difference-in-differences model. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Shocks in 2013 and 2014 are defined as June rainfall difference of that year with historical average below difference 20th percentile. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row.

Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A7: Effects on consumption dynamics

	(1) OLS: lower consumption pc in 2015 (descending)	(2) Probit (ME): lower consumption pc in 2015 (descending)	(3) OLS: predicted to descend	(4) Probit (ME): predicted to descend	(5) OLS: predicted to descend but did not	(6) Probit (ME): predicted to descend but did not
Village received cash	-0.0767**	-0.0768**	-0.0481	-0.0502	0.0456**	0.0458**
	(-2.30)	(-2.30)	(-1.40)	(-1.47)	(2.39)	(2.27)
Observations	1682	1682	1682	1677	1682	1682

t statistics in parentheses
OLS estimation, sample of households below the (baseline) PMT threshold and surveyed at baseline. Lower predicted consumption is estimated from control group households (see text for methodology description). Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table A8: Land, PSM-DID estimation, heterogeneity by exposure to shocks

	(1) Cultivates land	(2) Land area cultivated (ha)	(3) Number of fields	(4) Area rented (ha)	(5) Area rented out (ha)	(6) Built something against erosion
Cash Transfers (CT), 2015	0.0130	0.112	0.0179	0.0673	0.0410	0.0142
	(1.29)	(0.56)	(0.16)	(1.33)	(0.98)	(0.50)
Drought in 2014 (p20) (shock)	-0.00157	0.480*	-0.0458	0.0581	0.0484	-0.0420
	(-0.10)	(1.84)	(-0.24)	(1.13)	(1.06)	(-0.81)
Cash Transfers, 2015 * drought in 2014 (p20)	-0.0175	-0.794*	-0.174	-0.0742	-0.0474	-0.0429
	(-1.00)	(-1.78)	(-0.76)	(-1.21)	(-0.97)	(-0.83)
Observations	2192	2192	2192	2192	2192	2192
Mean in control	0.968	3.342	2.126	0.128	0.067	0.066
Median in control	1.000	3.000	2.000	0.000	0.000	0.000
CT+CT*shock=0	0.761	0.088	0.455	0.855	0.822	0.525
shock+CT*shock=0	0.245	0.422	0.302	0.740	0.981	0.084

t statistics in parentheses

Estimation of a matched difference-in-differences model. Beneficiaries of the cash transfer program are matched to households in control villages based on PMT variables. Shocks are defined as rainfall in June 2014 below the 20th percentile of the historical average. The last two rows of the table show the p value of a Wald tests for the sum of the coefficients in the 1st and 2nd row, respectively 2nd and 3rd row. Randomization strata fixed-effects are included (stratification based on commune and nomadic status). Standard errors are clustered at the village level.

* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Figures

Figure 1: PMT score and beneficiary status (actual and predicted)

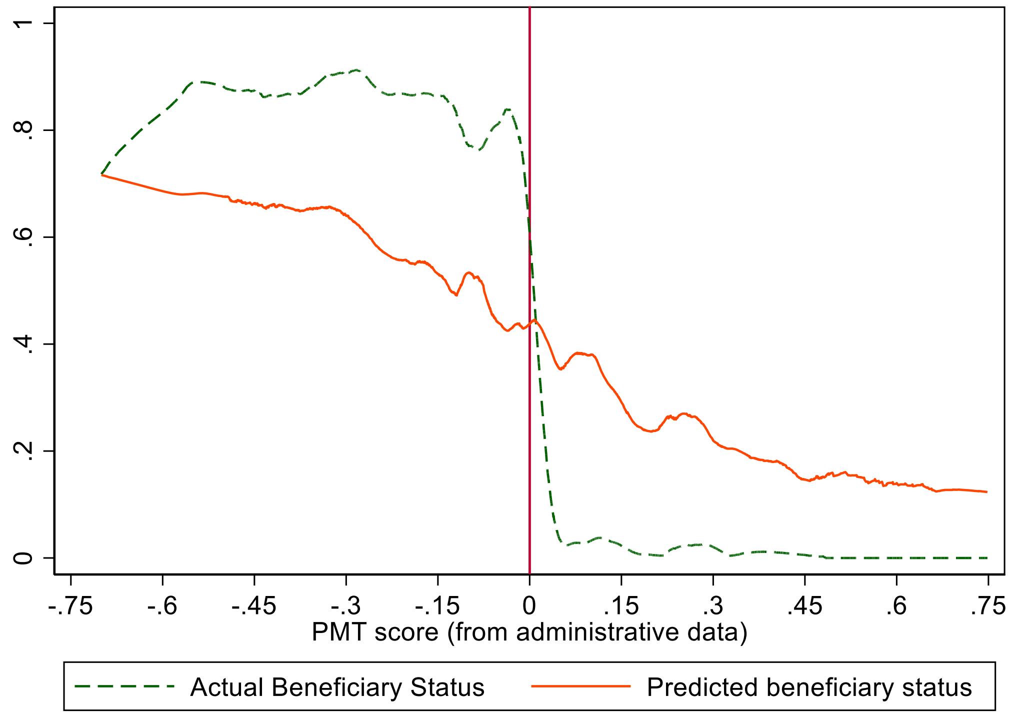
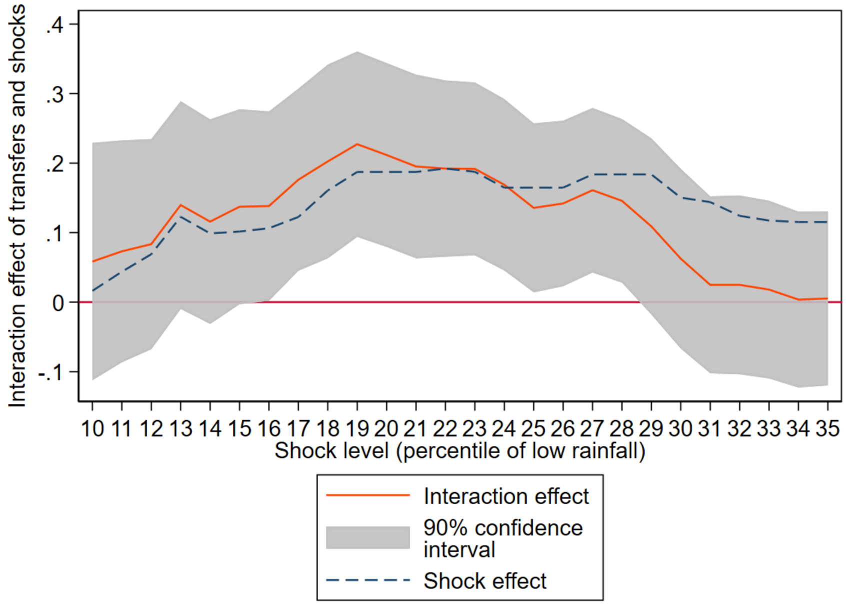


Figure 2: Effect of cash transfers on welfare by exposure to shocks, by rainfall deficit percentile

Panel A: log per capita consumption



Panel B: Food consumption score

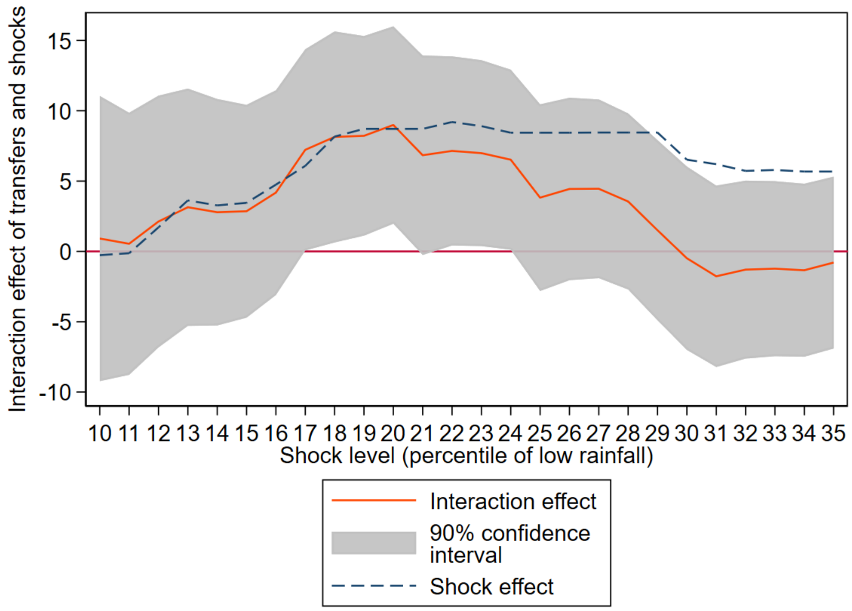
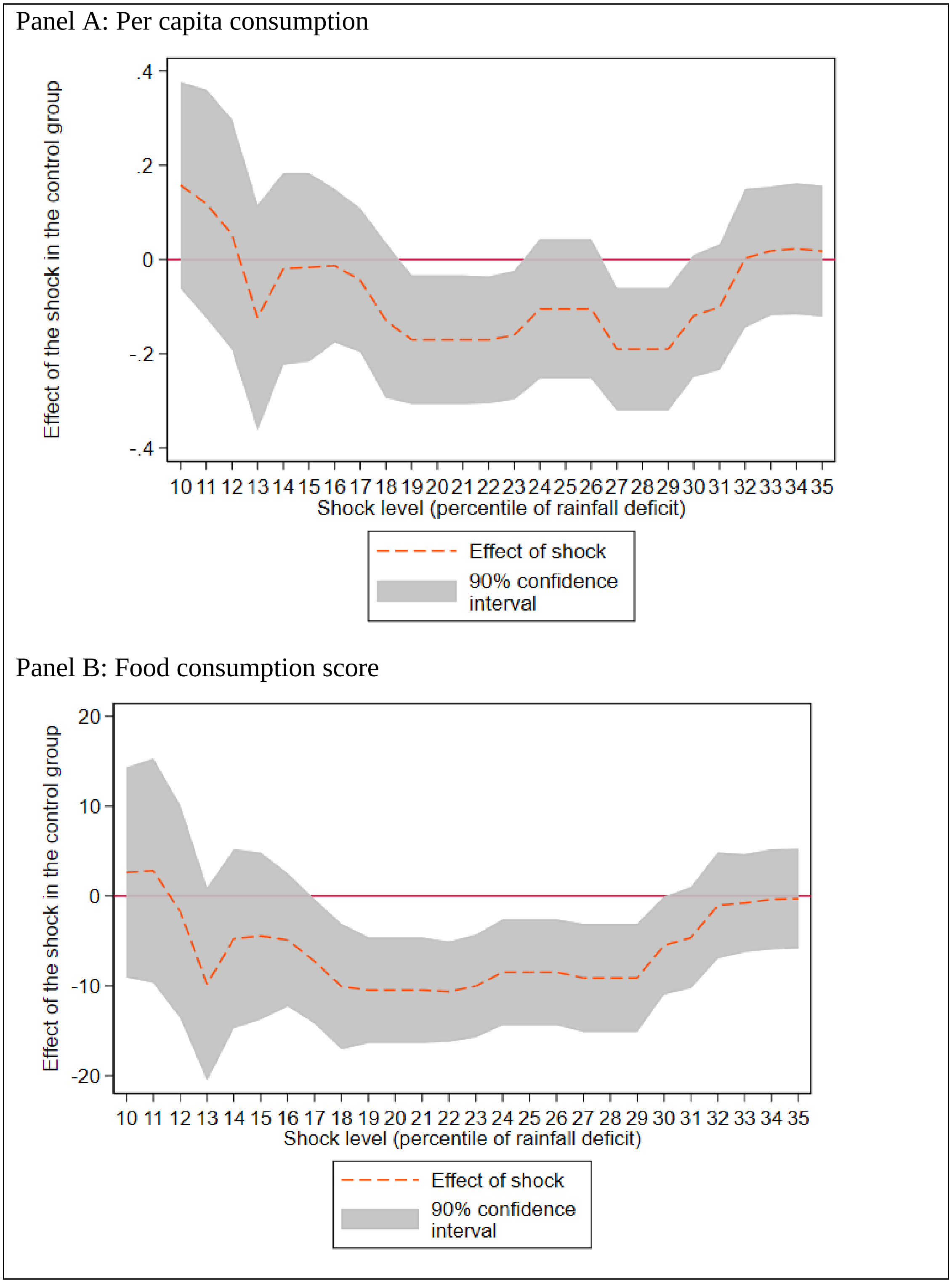
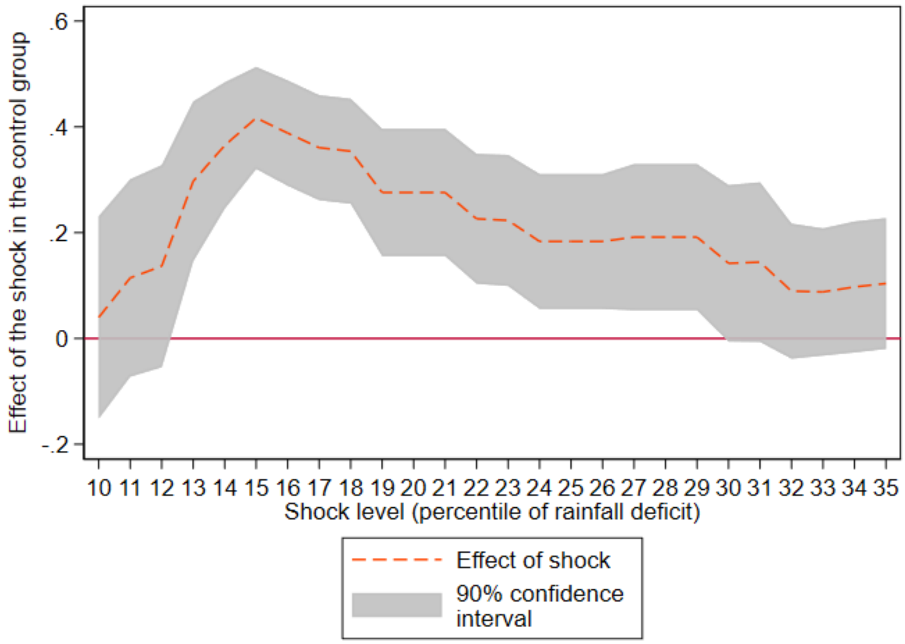


Figure A1: Effect of shocks on welfare in the control group, by rainfall deficit percentile



Panel C: Self-reported drought shocks



Panel D: Millet / sorghum production

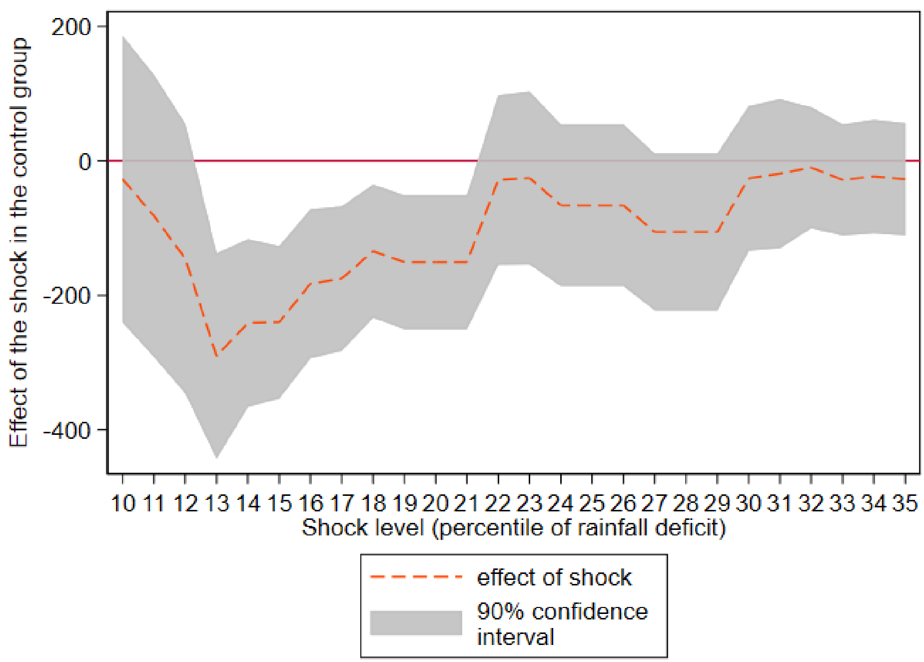
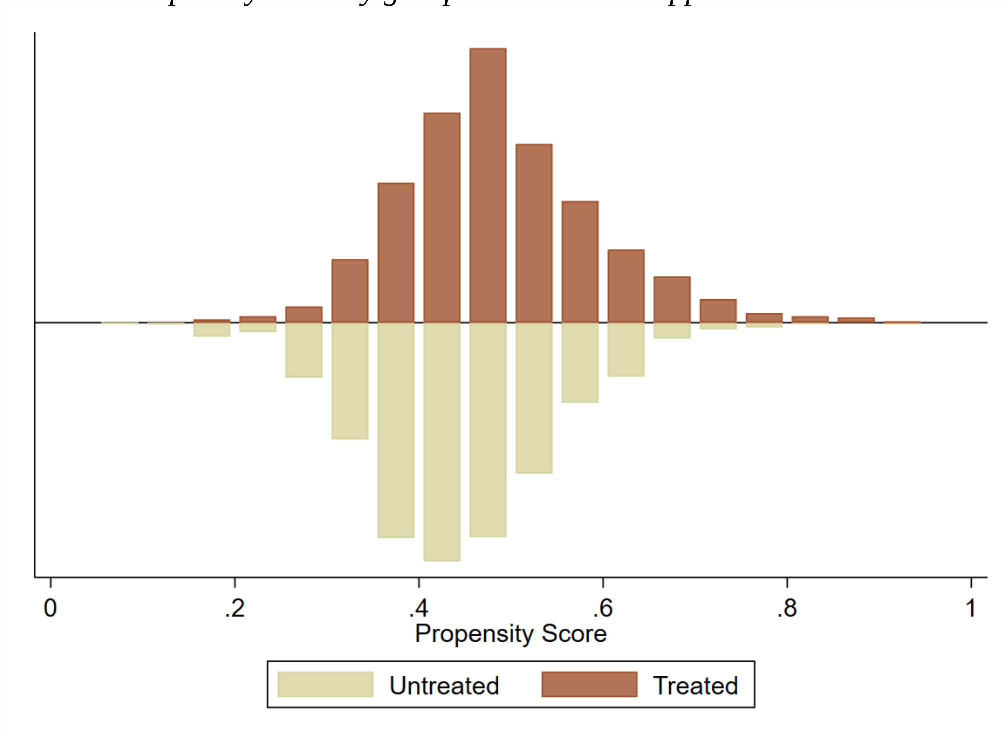


Figure A2: Quality of Propensity Score Matching

Panel A: Propensity score by group and common support



Panel B: Percentage bias before and after matching